Thank you for the careful review of our paper and your thoughtful suggestions. We hope that you will find our responses and the corresponding revisions for the original manuscript satisfactory. Please find below your comments/suggestions (bold) and our responses with manuscript changes indicated in italic.

Positive points
o Figures are legible and have appropriate font size.
o Sentences are clear and properly structured.
o I think that the authors description of the many sensor and algorithm upgrades will add value to the literature since these are often difficult to track down and are poorly documented.
o A great novelty of this article includes comparing temporal estimates of cloud cover – from radar, ceilometer and MPL - to domain estimates of cloud cover – from TSI trying to assess the impact of field of view.

We are delighted that the Reviewer highlighted these positive points.

Major Comments:

o 1) Confusing uses of the terms "cloud fraction" and "cloud cover" and other derivatives - The term "cloud fraction" is most commonly used to represent the amount of clouds present at different levels in the atmosphere and is most often presented as a profile. Here, where the authors effectively refer to the projected area of clouds at the surface, I think the term "cloud cover" would be more appropriate. - Here, it would also seem appropriate to introduce two district cloud cover concepts: 1)"Domain cloud cover" which would be the number of cloudy pixels in each TSI image relative to the total numberof pixels in each TSI image. This definition would be closest to what a large-scale model would simulate/report and is what we would ideally like to measure (i.e., "truth") and 2) "Temporal cloud cover" which is the number of cloudy pixels in time series relative to the total number of observations collected over a defined time period (e.g., in ceilometer or radar time-series over 30-min or in a TSI pixel over 15-min).

We agree with the Reviewer that clarifications are needed for different estimates of cloud cover. Both "cloud fraction" and "fractional sky cover" are widely accepted terms for describing cloud cover (e.g., Qian et al., 2012 and references therein), while the term "domain" frequently defines a volume with specified grid spacing and number of vertical levels for model simulations (e.g., Berg et al., 2013). Therefore, we prefer to keep these two conventional terms in our paper. To address valuable comment from the Reviewer regarding their "temporal" and "areal" representations, we have modified the abstract slightly (lines 15-18), clarified introduction of these terms in the Introduction (Sect. 1, lines 46-51), and added a reminder of terminology to the Results and Discussion (Sect. 4.2, lines 270-272).

--Abstract (lines 15-18): *Enhanced observations at this site combine the advantages of the ceilometer, micropulse lidar (MPL) and cloud radar in merged data products. Data collected by these three instruments are used to calculate narrow-FOV cloud fraction (CF) as a temporal fraction of cloudy returns within a given period. Sky images provided by TSI are used to calculate the wide-FOV fractional sky cover (FSC) as a fraction of cloudy pixels within a given image.*

--Section 1, (lines 46-51):
There are two conventional measurement-based estimates of cloud cover: (1) cloud fraction (CF) obtained from zenith-pointing narrow-FOV observations and defined as the fraction of time when a cloud is detected within a specified period, and (2) fractional sky coverage (FSC) obtained from wide-FOV observations and defined as the fraction of cloudy pixels in a sky image. Note that FSC is similar to that

estimated by a cloudy-sky observer (e.g., Henderson-Sellers and McGuffie, 1990; Kassianov et al., 2005; Long et al. 2006).

--Section 4.2 (lines 270-272): *Recall that the CF obtained from lidar-radar observations with narrow FOV represents a transect of a cloudy sky along wind direction, while FSC acquired from wide-FOV TSI data defines an area of cloudy sky. Both the CF and the FSC are widespread measurement-based estimates of cloud cover.*

o 2) The first goal stated by the authors is very similar to work undertaken by Kennedy et al. 2014. The authors first goal reads: "1) Have significant changes in the observations of ShCu cover occurred at the SGP site due to instrumental and algorithmic upgrades?". While I can appreciate that the current work tackles an extended dataset, to further add value, I wish it also went into more details about what are the exact algorithm changes or sensor upgrades responsible for the observed differences. For instance:

- Given what Kennedy et al. 2014 stated about the radar: "sudden change in CF occurs around the time the radar was upgraded, suggesting that this decrease is tied to hardware sensitivity or scanning strategy changes." The current study could quantify the additional amount of cloud detected solely by the radar sensitivity increase following the change from the MMCR to the KAZR.

- Given what Kennedy et al 2014 reported about the MPL: "Addition of the MPL increases the 14-year average CF by 9 %, mainly through an increase in optically thin high clouds year-round, and mid-level clouds during the summer months." In the current study, what can the authors say about the relative importance of such a sensitivity boost relative to the number of MPL false classification for ShCu. What exact changes were implemented following 2011 to improve the MPL cloud mask and can you recommend any further algorithm modification which could help mitigate the number of false ShCu detections?

The Reviewer is right that the algorithm changes and/or sensor upgrades are interesting topics, but their detailed discussion is beyond the scope of our current work.

However, we have expanded and clarified several important issues associated with these important topics.

- The radar-related changes and upgrades: Appendix A summarizes the instrumental and algorithmic changes of the radar. The primary limitation of the radar is the difficulty of observing optically thin clouds with low liquid water content (LWC), such as continental ShCu. The radar upgrades have primarily decreased the dwell time resulting in an increased number of pulses averaged. However, these upgrades very likely have not improved detection of the optically thin clouds. We have added a clarification regarding the sensitivity of the radar to cloud optical depth. (Section A.2, lines 463-465; lines 477-479).

- The MPL-related changes: The original cloud mask involved a signal-to-noise threshold based on cloud droplet scattering *(Campbell et al. 1998)*, whereas the updated cloud mask incorporated information on different scattering properties of aerosol and clouds (e.g., Wang and Sussen 2001). The corresponding clarifications have been added (Section 4.1; lines 254-258) and (Section A.3, Lines 481-483).

The specific changes are documented below.

--Section A.2 (lines 463-465):

Many continental cumuli are optically thin (liquid water content less than 50 g/m2) and have small droplets. As a result, the cloud radars (both MMCR and KAZR) are not able to "see" the majority of these clouds (e.g., Chandra et al. 2013; Lamer and Kollias, 2015).

--Section A.2 (lines 477-479)
Cloud radars (both the MMCR and KAZR) are known to miss detection of ShCu with small droplets compared to the ceilometer. For example, Chandra et al (2013) have found that the MMCR misses the majority of continental ShCu observed at the SGP site.

--Section 4.1 (lines 254-258): *Though a number of differences exist, the incorporation of MPL data (below 3 km) in the original cloud top height retrieval would increase number of detected cloud tops compared to those retrieved from the radar data alone for the initial period (2000-2010). Reliance only on the radar data for cloud top detection in the updated algorithm would result in fewer cloud top height detections and therefore a lower CF (see Sect. A.4 for more details).*

--Section A.3 (lines 481-483):
The original cloud mask involved a signal-to-noise threshold based on cloud droplet scattering (Campbell et al. 1998), whereas the updated cloud mask incorporated information on different scattering properties of aerosol and clouds (Wang and Sussen 2001). … It is expected that the updated cloud mask would improve cloud returns.

O 3) I think the second goal of the authors should take the forefront as it tackles something that remains poorly documented in literature. As it reads in the manuscript, the authors second goal is: "2) what is the impact of FOV configurations on hourly and sub-hourly observations of ShCu cover?". Taking it a step further I would be curious to know:
- Can narrow-field of view sensors be used to estimate a cloud cover representative of a domain?
- if so under what circumstances (e.g., strong horizontal wind, high cloud cover, ect.)?

In contrast to simulation-based studies, we use observations to quantify the impact of instrumental FOVs on cloud cover estimates. However, we should clarify that areas "seen" by ground-based instruments with narrow- and wide-FOV at the cloud base height are much smaller than areas (~ 30 km) considered in simulation-based studies (e.g., Oue et al. 2016) For example, TSI with 100-deg FOV "sees" a moderate area (~ 2.4 km diameter) at typical cloud based height (1km).

A clarification of differences between our approach and previous studies is included in section 4.2 (Lines 278-284):

In particular, the previous model studies (Astin et al., 2001; Berg and Stull, 2002) have demonstrated that the cloud cover obtained from the transect measurements mimics the area-averaged cloud cover for non-organized (e.g. random) cloud fields well if the sample size is relatively large (or numerous individual clouds are sampled). Recently Oue et al. (2016) have showed that 10 or more ceilometers equally spaced across a 25 km width in the cross-wind direction are required to estimate the simulated cloud cover in the small (30 km) domain. Certainly, the number of ceilometers, their locations and averaging time required for an accurate estimation of the cloud cover depend on the spatial arrangement of clouds and wind speed.

With this clarification in mind, we respond to the Reviewer's questions by explicitly stating that we interpret the term "domain" in the question to be an area "seen" by TSI with the 100-deg FOV at the cloud base height.

1) **Can narrow-field of view sensors be used to estimate a cloud cover representative of a domain?** Our results (Section 4.1) suggest that a narrow-FOV data from ceilometer as compared with those from lidar and radar can be used to estimate wide-FOV FSC reasonably well, on average, in terms of bias (Table 3), slope and intercept of linear regression (Table B2), and correlation coefficient (Table B3). For example, the correlation coefficient values for sub-hourly and hourly time scales are 0.83 and 0.89, respectively (Table B3). It should be emphasized that about 30-34% of the corresponding CF-FSC comparisons still have difference greater than 0.1 (section 4.2, lines 301-303). The level of agreement between the narrow-FOV CF and wide-FOV FSC depends on several factors described below (the second question).

2) **if so under what circumstances (e.g., strong horizontal wind, high cloud cover, ect.)?** Several factors, such as wind speed and spatial arrangement of clouds, determine whether or not the narrow-FOV CF and wide-FOV FSC are comparable, on average. The spatial arrangement of clouds (e.g., organized versus non-organized spatial distribution) defines representation of a 1D transect along a wind direction of a 2D cloud field for a given area of interest, while the wind speed determines the number of sampled clouds (or sample size). The "quick-look" tool (Section 4.3) can be used for characterization of the spatial arrangement of clouds while available information on the wind speed can be used for estimation of the sample size, and therefore for assessment of the wind speed impact on the CF-FSC comparison.

To address these points in the manuscript, two new paragraphs have been added. The first new paragraph discusses possible applications of the "quick-look" tool to assess the impact of cloud field organization on agreement between narrow- and wide-FOV observations (Section 4.3, lines 365-375). The second new paragraph describes the impact of wind speed on the agreement of narrow- and wide-FOV observations (Section 4.2, lines 306-3017). A new Figure 6 has been added to illustrate the concepts discussed in the second new paragraph.

The new paragraph discussing the impact of cloud field organization on the agreement of narrow- and wide-FOV observations (Section 4.3, lines 365-375):

"There are two main expected applications of the introduced "quick-look" tool. The first potential application is a classification of spatial organization of cloud fields using, for example, cross-wind cloud field variability (e.g. peaks and valleys in Fig. 7b) and within-lane variance of cloud amount (e.g. vertical bars in Fig. 7b). Numerous images generated by the "quick-look" tool (e.g., Figure 8b) for the extended period (2000-2017) can be considered as a valuable training dataset for machine learning with focus on automated detection of desired features of the cloud fields (e.g., "cloud streets") and unwanted contaminations of TSI images (e.g., Figure 9). Second potential application is a visual inspection of the generated images for a given period of interest (e.g., a short-term field campaign) to check for the impact of instrumental detection differences and cloud field organization on the observed cloud amount. Visual inspection may be feasible given a limited number (about 40) of ShCu events annually during the warm season. For example, a spread of the lane CFs (gray region in Fig. 8c) gives an idea about the cross-wind cloud field variability within a given FOV, and thus aids in understanding the difference between cloud amounts obtained from the narrow- and wide-FOV observations."

The new paragraph discussing the impact of wind speed and the sample size on the agreement of narrow- and wide-FOV observations (Section 4.2, lines 306-317).

"The effective spatial area sampled by either narrow or wide FOV instruments is a function of both sampling duration and wind speed. High wind speed in comparison with low wind speed (1) increases sample size for a given period and (2) tends to organize horizontal arrangement of clouds (e.g., Weckworth et al. 1999, Atkinson and Zhang 1996). These two factors associated with sample size and

spatial arrangement of clouds should be considered when differences between cloud cover obtained from narrow- and wide-FOV observations as function of wind speed are considered (Fig. 6). In particular, Figure 6 illustrates that both CF-FSC and "CF-like"-FSC differences are reduced noticeably as the wind speed increases from 1 m/s to 3 m/s, and continue to reduce slightly as the wind speed grows up to 11 m/s. The CF-FSC and "CF-like"-FSC differences obtained at a higher wind speed (above 11 m/s) should be considered with caution due to limited number of the corresponding cases with high wind speed (e.g., fewer than 100 cases for 60-min time average). The increased sampling area associated with increased wind speed does not necessarily result in an improved agreement between the narrow- and wide-FOV observations for both hourly and sub-hourly observations due to the impact of wind speed on cloud organization."

o 4) Did the authors consider the effect of horizontal wind speed in the comparison of domain cloud cover and temporal cloud cover? I would expect that higher horizontal wind speeds advect clouds more rapidly such that, under higher wind speeds, shorter time periods of narrow-field of view measurements would be required to capture the CF observed by the wider field of view TSI.

To address your important questions, a new paragraph regarding the wind speed and the sample size (Sect. 4.2, lines 306-317) together with a new plot (Figure 6) have been added. This paragraph has been included in our reply above.

o 5) The spatial analysis of TSI cloud mask is very interesting but I had to read the article twice to understand where is fits in with the other cloud fraction definitions. What would make this clearer for me would be to state that the TSI "lane by lane" cloud fraction estimates are effectively temporal cloud fraction estimate (and not FOV or domain cloud fraction estimates) and that each lane can be interpreted as a time series observed by a narrow field of view sensor. I would also perhaps bring information about this lane by lane methodology and information about the radar wind profiler to section 4.3 where you describe your tool.

Thank you for the valuable suggestions. We have moved the description of the lane-by-lane methodology and the radar wind profiler (previous section 3.5) to section 4.3, added clarification to section 4.3, and changed the labels on Figure 7 from FSC to CF.

The clarification (Section 4.3; lines 328-330)
Each pixel in the averaged image can be interpreted as a 15-min CF measurement from a narrow-FOV sensor. The variability of CF in the cross-wind direction can indicate the possible influence of cloud field organization on cloud cover estimates provided by narrow-FOV observations.

Also, the vertical axis label for Figure 7b has been changed from FSC to CF for consistency with the above changes.

o 6) Clarifications regarding the impact of insects on ShCu top detections - Multiple studies have reported that the presence of insect hinders the radars ability to accurately detect cloud top. I think more information is needed here about how insect contamination is handled in ARSCL both pre and post 2011 where the authors hint that the MPL stopped being applied in the boundary layer. This could offer an alternative explanation to the changes in radar-lidar CF post 2011 where the increase in radar detected cloud top could be due both to the KAZR being more sensitive than the MMCR and to the KAZR insect filtering having changed such that more insect returns are misclassified as cloud tops. If both effects are in play, then I would like to see their relative importance quantified.

We agree that insects can contaminate accurate determination of cloud boundaries by radar. However, accurate cloud top height retrievals by radar is not required in our analysis because a simple threshold is used to determine the presence of ShCu. Moreover, cloud base height estimation involves lidar observations (both ceilometer and MPL) which are not impacted by the presence of insects,

Text has been added to clarify this point: (Section 3.1, lines 141-144)
Insect contamination may contribute to significant uncertainty of the radar-based retrievals of cloud boundaries. Therefore, our analysis employs a semi-quantitative threshold approach when using the cloud top heights. This approach is less sensitive to the insect contamination.

o 7) The idea of compensating bias introduced on Page 8 “introduction of compensating errors using the cloud top height criteria in the updated merged lidar-radar product.” needs clarification.
- If I understand correctly the hypothesis is that in the 2000-2010 period the MPL was overly sensitive to aerosols leading to a CF overestimation while the MMCR was underly sensitive to cloud leading to a CF underestimation hence the compensating bias

Thank you for pointing this out. For the later sub-period (2010-2017), the merged cloud radar-lidar product relies on the “shallow” (< 3 km) radar data instead of the combined MPL-radar observations for determining the cloud top. The radar misses a substantial fraction (about 30%) of ShCu, therefore the cloud top height (below 3 km) is very likely to be missed. Meanwhile, the merged lidars (ceilometer and MPL) data are used to detect the cloud base height and exhibit higher CF than that from the ceilometer alone. A compensating error could potentially arise from the over-detection of clouds in the merged lidar data with the under-detection of cloud from the radar observations. The RMSD for the CF including cloud top heights for the later sub-period (2010-2017) is higher than those for the CF obtained from ceilometer alone (even for near-zero bias). This indicates that the instrument detection differences in the merged lidar-radar product contribute mostly to the CF uncertainty.

o There are gaps in the literature review
- Beyond the few studies cited on Page 2 line 30, others before have attempted to assess the representativeness/reconcile multiple cloud fraction measurements (e.g., Dr. Mariko Oue work with scanning cloud radar or Dr. Steve Schwartz work with photography or Dr. Wei Wu work with ISCC).
- Some references are missing for the bibliography (e.g., Tatarevic and Kollias, 2015)
- Some references are to meeting abstracts rather than to the published journal articles (e.g., Lamer et al. 2017 abstract work has since been published in GMD)
- Some references are miscited (e.g. Chandra et al., 2013, Zhang and Klein, 2010, 2013 and Lamer and Kollias, 2015 do not show any model-observation comparison).

We thank the Reviewer for the careful editing and helpful suggestions. We have made the following adjustments to the manuscript.

Introduction (Lines 69-71)
- We have added Oue et al. (2016) reference along with two citations in the text.
Moreover, sampling of LES-generated cloud fields by a virtual instrument can be a helpful way to reconcile debated differences between the retrieved and predicted values of cloud cover (Oue et al., 2016).

Section 4.2 (lines 278-284)

In particular, the previous model studies (Astin et al., 2001; Berg and Stull, 2002) have demonstrated that the cloud cover obtained from the transect measurements mimics the area-averaged cloud cover for non-organized (e.g. random) cloud fields well if the sample size is relatively large (or numerous individual clouds are sampled). Recently Oue et al. (2016) have showed that 10 or more ceilometers equally spaced across a 25 km width in the cross-wind direction are required to estimate the simulated cloud cover in the small (30 km) domain. Certainly, the number of ceilometers, their locations and averaging time required for an accurate estimation of the cloud cover depend on the spatial arrangement of clouds and wind speed.

- We have added Tatarevic and Kollias (2015) to the bibliography,

Conclusions (line 408)
We have replaced the Lamer et al. (2017) reference with Lamer et al. (2018) GMD article, and adjusted the bibliography accordingly.

Introduction (lines 52-53)
- We have removed the citations to Chandra et al., 2013, Zhang and Klein, 2010, 2013 and Lamer and Kollias, 2015 for model-observation comparison, and inserted Zhang et al., 2017 and Endo et al., 2019.

o 9) Although I understand that there are many ways to organize a methods section and that we all prefer to receive information in different sequences, for me, the layout of the data and methods section was confusing.
- I would rather the authors merge the data and methods sections which go hand in hand and preface such a section stating what quantities they are after 1) Identification of cumulus cases, which requires cloud top height and cloud cover estimates 2) Temporal cloud fraction, which will be obtained from ceilometer, ceilometer+MPL, and ceilometer+MPL+radar cloud base height time series 3) Domain cloud fraction, which will be obtained from TSI using different angular domains 4) Providing context using horizontal wind direction and TSI lane-by-lane decomposition

We thank the Reviewer for this helpful comment. Indeed, during development of this manuscript we have attempted many possible organizational approaches to communicating the data and methods used to produce this dataset. One challenge with the approach that the Reviewer suggests is the interdependent nature of these data. For example, the natural first quantity of the paper, as you list above, is the identification of ShCu events. However, this method requires TSI, ceilometer, and lidar/radar data (items #2 and 3 above). Thus we settled on the current organizational structure of the data and methods sections.

While the current layout may be somewhat cumbersome to read as prose, we believe this structure will be most conducive for readers to access the data and perform their own analyses in extension of this work.

o 10) Clarification are needed when it comes to ShCu case selection
Thank you for your careful attention to detail in the review of this paper. We address each point independently below.

- Page 4, Line 3: Which observations are used in the ARM Shallow Cumulus data product to identify ShCu cases?

The reference to the new Shallow Cumulus data product has been added for clarification of the observations used in our study. (Section 2, lines 108-110)
The newly released ARM Shallow Cumulus data product (Data Reference: Shi et al., 2000) identifies times of ShCu from lidar / radar cloud boundary heights and includes FSC from TSI observations (Lim et al., 2018).

- Table 1 somewhat helps in understanding which portion of the cloud field is of interest. However, I do not see a reference to Table 1 in Sect. 3.1. where I would expect it.

We respectfully note that Table 1 is referenced in the third sentence in Sect. 3.1. To facilitate readability, we have added another reference to it later in this paragraph. (Line 146)

- Figure 2 shows cloud fractions ranging from 0 to 1, do you consider overcast conditions to be ShCu?

The FSC obtained from 100-deg FOV TSI data was used to determine clear-sky or overcast conditions. Only partly-cloudy conditions (0.05<FSC<0.95) were considered in our study. This is stated in Section 3.4 (line 185), and the captions of Figure 2, Table B1, Table 2, Table 3, and Figure B1.

- Page 4, Line 8: Why did you chose to: "additionally extending the start and end-times by 1 hour each.". This could include periods presenting deeper clouds or cloud aloft.

The text describing Shallow Cumulus event detection was revised to improve clarity. (Section 2, lines 134-138)
The extension allows for more accurate determination of the start- and end- times of the event on the finer time scale of the TSI FSC (15 min). Quality control procedures (Sect. 3.4) are used to censor multi-layer clouds and clear sky conditions on the 15-min and hourly observations of cloud cover.

- Table 1. Why is a minimum cloud base height threshold applied to most CF estimates? If insects and clutter have been properly filtered from the radar data, I cannot see why this could be necessary.

This helps additionally screen out smoke from agricultural burning of biomass, which happens several days each warm season. It would eliminate fog too, which has never been an issue.

- Table 1 What is the value of estimating a CFtot if the cases discussed are purely single layer ShCu? Shouldn't all cloud observed have tops and bases below 3 km?

The ShCu data product includes ShCu cases with overlying cirrus. The TSI cannot differentiate low clouds from cirrus, which can be sporadic with short duration. The selected cases were required to have at least 2 hours without cirrus.

o 11) Pertinent information missing in the various definitions of cloud fraction

- Page 3 Line 15: "Appendix A contains pertinent information for their application". I believe all pertinent text should be in the main text and the appendix should be reserved for details. I am especially wanting to know how the ARSCL reports cloud top height when both the radar and MPL

are used (in the 2000-2010 period) since this is very relevant to the sensitivity versus insect detection compensating effect.
Appendix A provides detailed information about data streams and data pre-processing. This is valuable background knowledge for researchers.

The issue of cloud top height observations in the earlier (2000-2010) and later (2011-2017) periods is indeed interesting and relevant and discussed in full in the Conclusions. In particular, the corresponding discussion under the first research question "*1) Have significant changes in the observations of ShCu cover occurred at the SGP site due to instrumental and algorithmic upgrades?"* is included. (lines 383-394)

Minor Comments:

1) In the future, when submitting articles for review, please number the lines continuously rather than restarting the numbering process on each page.

We agree that continuous line numbering is preferable - The revised manuscript uses continuous line numbering.

2) The abstract is very "number focused" and could benefit from including more "conclusions", for instance, the abstract does not provide information about which sensor upgrade had the largest impact on the cloud cover estimates or about the fact that cloud field organization (e.g., cloud streets) parallel to the horizontal wind direction can create large biases between narrow and wide field of view cloud cover estimates.

We have addressed the first point by adding a statement to the abstract regarding the algorithmic upgrade that had the largest impact on the cloud cover estimate (lines 26-27), and we have addressed the second point regarding the potential bias in observations due to cloud field organization (lines 32-33). The specific edited text is below.

Abstract (lines 26-27): *The strong period dependence of CF obtained from the combined ceilometer-MPL-radar data is likely due to increased reliance on the radar for cloud top height returns.*

Abstract (lines 32-33): *The influence of cloud field organization, such cloud streets parallel to the wind direction, on narrow- and wide-FOV cloud cover estimates can be visually assessed.*

3) Page 1 Line 22: What is meant by "mean cloud cover"

The term "mean cloud cover" has been replaced with the term "multi-year mean cloud cover" (Abstract, line 23)

4) Page 2 Line 2: Missing some "the"

Thank you - this has been changed. (Section 1, line 40)

5) Page 2 in a few places. I would suggest using the word "variability" instead of the word "changes" when referring to the cloud field

Thank you - this has been changed in the Introduction, lines 43 and 45.

6) The acronym for the Ka-band ARM Zenith Radar should be entirely capitalized (i.e., "KAZR" not "KaZR")

Thank you - all instances have been changed.

7) Page 2 Line 18: What do you mean by "consistent"?

The word "consistent" has been removed to avoid confusion. (Page 2, line 56)

8) Page 2, Line 18: Zhang and Klein 2013 used 13 years of ARSCL data, the sentence as you have it constructed is somewhat misleading as it suggests that they used 20 years of data. It would be more appropriate to state that previous studies have used ARSCL (cite here) and this data record is now reaching 20 years in length.

The text has been changed to indicate that the ARSCL data have been available since Nov. 1996 to avoid confusion. The revised sentence reads:

(Introduction, lines 56-57) *A merged lidar-radar data product is available from Nov. 1996 to the present at the SGP site and has served as a basis for developing ShCu climatology at the SGP site …*

9) Page 2 Line 23: Following my suggestion above "Areal cloud cover" would become domain cloud cover.

We've elected to maintain our conventions. Please see the discussion in the first question of this review. Thank you for your suggestion.

10) Page 5 Line 1: Given that the radar can be affected by insects, I would avoid using the word "reliably".

Thank you for this suggestion. The wording of these sentences was changed in response to major comment number 6.

11) Page 4, Line 4: Shouldn't "ShCu cloud coverage" read "ShCu cloud periods"?

Yes, thank you. This has been changed to *times of ShCu*. (Section 2, line 109)

12) Figure 2 Panels a and b are missing a legend

Note that this is now Figure 1. The caption has been updated. Note that the 1D histograms were changed to line plots in response to a separate Reviewer comment.

Figure 1 (caption): *merged ceilometer-MPL (blue) and merged lidar-radar (red)*

13) Figure 2 c and d and all figures of this style are missing colorbar labels

Thank you. The captions for figures 1, has been updated to include specific mention of the colorbar.

Figure 1 (caption): *joint histograms (counts)*
Figure 2 (caption): *Color scale represents counts in increments of 10.*

14) Page 4, line 31: "This method has the advantages of low missing data due to multiple instruments used and limits the vertical extent of clouds." Please rephrase. Using a cloud top detection criteria does not "limits the vertical extent of clouds".

Thank you for this suggested clarification. The relevant passage has been changed:
(Section 3.1, lines 148-149) *This method has the advantages of low missing data due to use of multiple instruments and incorporates information about cloud top height consistent with the definition of shallow convection.*

15) Page 9 line 4: Add "altitude" after "1.5 km"
We respectfully note that the sentence already reads "*1.5 km cloud base*", which employs the correct terminology. (Section 4.2, line 272)

16) Page 2, Line 29 "In addition, long-term averages of CF obtained from merged ceilometer-MPL data tend to be larger than FSC (Boers et al., 2010; Qian et al., 2012; Wu et al., 2014; Kennedy et al., 2014), indicating a potential consequence of instrument-dependent cloud detection differences." Could this difference not also be attributable to FOV differences? If so, please add this caveat.

Long-term averages are monthly and yearly long, and have sufficient sample size that reduce the impacts of limited FOV. A potential long-term impact of FOV configuration on cloud cover estimates could be associated with cloud sides visible at moderate zenith (viewing) angles. However, one would then expect that wide-FOV FSC would exceed narrow-FOV CF. The opposite is demonstrated by our analysis. This suggests that the obtained difference between narrow-FOV CF and wide-FOV FSC is mainly due to instrument-dependent cloud detection differences.

17) Page 1 Line 19 "We demonstrate that CF obtained from ceilometer data alone and FSC obtained from sky images provide the most similar and consistent cloud cover estimates: bias and root-mean-square difference (RMSD) are within 0.04 and 0.12, respectively."
According to your analysis of the impact of the "Field Of View (FOV)" performed by comparing the two TSI FOV, the averaging period of the narrow field of view sensor can affect the RMSD between cloud cover obtained by the narrow and wide FOV. Am I correct to understand that this result also applies to the comparison between the ceilometer or any other "beam" observation (e.g., radar, MPL) and the TSI? If so, I think the statement above should include information about the averaging time period used for the ceilometer in this comparison.

Thank you for the careful attention to detail. Yes, the "CF-like" aims to characterize the effect of FOV on cloud amount observations, since it is derived from the same instrument (the TSI) as the wide-FOV FSC. Indeed, the characterization of the RMSD between "CF-like" and FSC is 0.1 for 60-minute averages, and 0.15 for fine temporal scale (30-minute CF-like, 15-minute FSC). We accept your suggestion, and edit the text to read as follows:

(Abstract, lines 21-23) *We demonstrate that CF obtained from ceilometer data alone and FSC obtained from sky images provide the most similar and consistent cloud cover estimates: hourly bias and root-mean-square difference (RMSD) are within 0.04 and 0.12, respectively.*

18) Page 8 line 24: "Though a number of differences exist, the incorporation of MPL data below 3 km in the initial cloud top height retrieval algorithm between 2000-2010 but not the updated algorithm likely has a large impact (see Sect. A.4 for more details)." I think it would help the reader to explain if an overestimation or an underestimation is expected and why?

This sentence has been changed to read:

(Section 4.1, lines 253-257) *Though a number of differences exist, the incorporation of MPL data below 3 km in the original cloud top height retrieval would increase the number of detected cloud tops compared to those retrieved from the radar data alone for the initial period (2000-2010). Reliance only on the radar data for cloud top detection in the updated algorithm would result in fewer cloud top height detections and therefore a lower CF (see Sect. A.4 for more details).*